Estimating raindrop size distributions using microwave link measurements

by T. van Leth, H. Leijnse, A. Overeem and R. Uijlenhoet

Assessment:

This is the second time I review this paper and my overall impression of it remains rather negative. I'm particularly disappointed in the way the authors handled my previous comments. Some superficial changes were made but the really important issues regarding feasibility and validation remain the same (see below for more details). In their rebuttal, the authors say that "the suggestion to dig deeper into the experimental data is not helpful and further analysis over the whole experimental dataset would not yield meaningful results." I don't agree with this assesment and encourage the authors to reconsider their position. In particular, I don't think that there is enough scientific evidence to support the feasibility of the retrieval methods yet. The simulation study is interesting but highly idealized and far away from reality. Given that there is still no proper uncertainty/error analysis, it is hard to judge the soundness of the retrievals. Unfortunately, since the authors do not seem to be interested in performing more detailed and rigorous investigations, I cannot recommend publication at this point.

Main arguments against publication:

1. There is no rigorous and realistic assessment of the uncertainty affecting the retrieved DSD parameters (e.g., no error bars and no benchmark for comparing results). The simulation studies are performed in idealized conditions which do not reflect reality.

2. The presented evidence does not always match the conclusions/statements made by the authors. There are several inconsistent and contradicting sentences (see below). The general conclusion of the paper regarding feasibility remains unclear.

3. The writing is biased towards highlighting potential rather than providing a fair objective scientific assessment of feasibility and accuracy.

A. Feasibility

A1. The authors base most of their conclusions on a few, highly idealized simulation studies. But to me, these are of little practical and scientific value. In reality, there are serious issues due to the instability of the baseline, quantization and wet antenna attenuation which make the proposed techniques very unlikely to be ever applicable to commercial microwave networks. Indeed, Figure 5 shows that the relationship between the attenuation ratio and the value of mu is almost flat. To get a good accuracy on mu, one therefore needs a very high accuracy on the attenuation ratio. *"To achieve a non-convergence ratio of 10%, quantization errors of 0.001 dB would be required."* However, current accuracies are 0.1 dB at best, which is several orders of magnitude lower than what is actually required. Higher accuracies are unlikely to be ever available in commercial networks due to the high cost of measuring power more accurately and other technical limitations (e.g., additional uncertainty due to baseline and wet antenna).

A2. Figure 14b clearly shows that the attenuation ratios derived from actual data are extremely noisy and poorly correlated with the true attenuation ratios. And this is for a "good" case without any quantization noise. Sure, you can cherry pick a few decent retrievals in there. But these might as well be coincidences and there is not enough hard evidence to prove feasibility. Please consider more cases and/or perform a more systematic and rigorous assessment.

B. Validation and assessment:

B1. The approach used to validate the DSD retrievals using MOR, MAD and AD95 on N(D) and R is inadequate. At best, it's incomplete. On their own, these values don't mean anything! A proper validation requires a benchmark against which the reported performances can be compared. For example, if the goal is to retrieve the rainfall rate from the links, then you should validate against the alternative model of retrieving R through the power-law relation $A = aR^b$ (without any knowledge of the DSD). If your method does not perform better than that, then there is no skill in the retrieved DSDs for the rainfall estimation problem. Similarly, if your goal is to retrieve the Dm or mu values, then you should validate against the alternative model which assumes a constant value (e.g., the climatological mean). In any case, error bars and a rough estimate of the uncertainty affecting the retrieved quantities need to be provided!

B2. It is not 100% clear how MOR, MAD and 95AD were calculated. Please provide unambiguous expressions/equations for your performance scores and clarify the difference between the "normalized" and non-normalized versions.

B3. It would be good to show a few cases in which the retrievals failed in order to have a better understanding of the numerical issues involved and the type of measurements that cause the algorithm(s) to fail. Right now, the paper mostly focuses in highlighting good cases, which is only one side of the story.

B4. The sensitivity study in 5.3 is based on unrealistic assumptions. The use of an equal offset for both frequencies/polarizations is much too optimistic. In reality, the errors/offsets on the individual measurements are likely to be independent. Indeed, the final offset is the result of many error/noise terms from multiple factors such as electronics, baseline attenuation, wet antenna and quantization effects. By assuming the same offset for both measurements, you are dramatically underestimating the uncertainty affecting the attenuation ratios. Please use independent offsets during the sensitivity study or justify why you think it is appropriate to use correlated noise terms.

B5. Page 18, ll.16-17: Why don't you take the effect of noise into account in the simulations. Please explain!

B6. Page 13, Figure 7: There are important conditional biases in the retrievals of Nt and mu. But almost no explanations are given to what caused them. A more detailed discussion is needed to understand these results and how they affect the quality of the DSD retrievals.

C. Inconsistent and/or misleading statements:

C1. Page 1 (abstract): *"Simulations show that a DSD retrieval on the basis of microwave links can be highly accurate."* This is a strong statement that is not aligned with the evidence presented in the paper. In reality, the simulations show that even under idealized conditions, the retrievals can fail. Please reformulate.

C2. Page 25, ll. 8-9 the authors write that: *"This provides a hopeful perspective for the application to commercial networks."* However, this is not really consistent with the other statements made in the paper. For example, on page 21, l.7 it is said that *"This limits the prospective of successful application to current networks"*. On page 25, ll. 5-6, *"the links examined in this study lacked stability […] and wet antenna attenuation was an intractable problem"*. On page 25, ll. 11-13, *"The only way to apply such retrievals to currently operational unmodified link networks consistently is to install dedicated data-loggers at selected link locations to read out the analog signal directly, which might not be feasible."*

C3. Page 25, ll.2-3: *"No such problem exists in principle with regard to the phase difference; it is independent of any baseline as long as that baseline is indifferent to polarization."* Yes, but there is no

evidence that the baseline is actually indifferent to polarization and wet antenna attenuation. Please reformulate the sentence to avoid misunderstandings.

C4. Page 27 (conclusions): *"... we have shown that a DSD retrieval on the basis of multiple microwave link variables can be successful and highly accurate, but only when precise high-resolution records of received power are available."* This is a very misleading statement. Firstly, the conditions under which a retrieval can be made and the uncertainty affecting the retrieved values remain unclear (i.e., due to the lack of a proper uncertainty analysis). Secondly, what is really needed for a successful retrieval is a precise measurement of the "rain-induced attenuation" and not the "received power". That's a big difference because in practice, it is almost impossible to get a precise rain-induced attenuation estimate, even if you could measure the received power accurately. Please reformulate to convey the right meaning.

D. Others:

D1. The discussion about computation time is not really relevant. There are no real challenges associated with the numerical optimization techniques used in this study and real-time implementation would not be a problem. I suggest to shorten this part or remove it in favor of a more detailed uncertainty assessment.

D2. Page 4, section 2.2: The temporal resolution of the DSD data are missing. I assume it's 30s?

D3. Page 8, l.13: the figure number is missing

D4. Page 10, l.17: What do you mean by "real outliers?" As opposed to imaginary ones?

D5. Page 12, ll.1-2 *"These outliers do not seem to correspond with any … with high drop concentrations". Why? Can you elaborate?*

D6. Page 12, l.10: of the rain intensity ~~is~~ are given in Table 1

D7. Page 19, Figure 11: Please provide units for MOR, MAD and 95AD and specify what quantity is considered here (N(D) or N(D)/Nt?).

D8. Page 21, l.12: "This ~~is~~ can be attributed"

D9. Page 26, the threshold used to select DSDs for inferring the mu-lambda relationship is not what I call a "compromise". It's a fixed threshold imposed by the authors based on a previous paper without any justification or optimization. Please reformulate.

D10. Page 26: "Considering the small spatial scale of the measurements we considered and the high spatial correlations therein this is an acceptable loss". This sentence does not make any sense.

D11. Page 26-27: The discussion about the truncation of the gamma distribution on page is besides the point. The real issue is not the truncation but the fact that real DSDs are never perfectly gamma. Even if they were distributed according to a gamma at the point scale, the average DSD along the link path would be a mixture of gamma distributions with different shape parameters which is not a gamma anymore. To me, the whole discussion about the truncation issue seems to be a minor issue in this story. Instead of obsessing about it, the authors could provide more details about the sampling uncertainty affecting the retrieved DSD estimates or the sensitivity to the temporal resolution of the link data.

D12. Page 27: "… but the effects of this on high order moments is minute". Does not make any sense. Please reformulate.